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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Determination of time- and height-resolved volcanic ash emissions for quantitative ash dispersion modeling: the 2010 Eyjafjallajökull eruption" *by* A. Stohl et al.

Anonymous Referee #3

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The paper presents a method to determine time- and height resolved volcanic emissions by combining satellite observations from two different instruments with Lagrangian particle dispersion modeling. The presented approach accounts for the need to derive good estimates of the strength of volcanic emissions and their high variability in time and space (including the vertical dimension). It could help a lot to improve forecasts of volcanic ash concentrations in the atmosphere in case of volcanic eruptions in the vicinity of areas with high air traffic like the European continent. The paper is well written and interesting to read. However, some paragraphs are unclear and some of the presented results seem to be contradictory. This has to be ruled out before the paper can be published in Atmospheric Chemistry and Physics.

General

My main concern is that it remains unclear how the inversion algorithm determines the a posteriori emission profiles from satellite observations from two different space borne instruments and the FLEXPART model results. I know that the method was described in previous studies but here the algorithm acts as a "black box" which produces a presumably better estimate of the volcanic emissions than they could be derived by using the "standard" procedures that rely mainly on the observed plume height. What in particular confuses me is that the a posteriori emissions are lower than the a priori emissions AND the satellite observations. Fig. 7 shows that the columnar ash totals are higher in both satellite pictures than in the model results based on the a posteriori emissions. In the text it is written that the inversion algorithm leads to lower a posteriori emissions compared to the a priori estimate. If the satellite pictures show higher columnar ash totals than calculated by the model, how can they influence the emissions to go down? Figure A1 to A5 show that the a priori columnar values fit better to the SEVIRI observations than the a posteriori values.

It also needs to be explained how you get improved vertical profiles by adding information about the columnar ash values. I can imagine that it might be related to the different dispersion in different altitudes due to wind shear but it is not said in the paper and maybe I am wrong and there is additional information from the satellites used.

Specific comments

Title: You might think once more about it: Now it implies that the paper presents a way to determine emissions that may be used by others in their models. This is certainly true bout only half of what is shown here because you use these emissions in your model, too, and you draw conclusions from this subsequent model run (e.g. how much of the European air space was affected by ash concentrations above certain limit values.

You could e.g. replace the "for quantitative ash dispersion modeling" by "and their

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application in a quantitative ash dispersion model".

Abstract

Line 4: I do not see "dramatic" improvements. There are some larger differences in the a priori and a posteriori emission estimates, particularly in the initial phase of the eruption, but I did not see a comparison to independent observations that proves that there are "dramatically" better results with the a posteriori emissions.

Introduction

Line 26/27: "However, these relationships are subjective ...": Why are they subjective? Assumptions may be subjective but the method presented here also relies on a number of assumptions. I do not see a general difference between empirical relationships to estimate mass fluxes from plume heights and this method that would allow to call this method "objective" while others are "subjective". To me, the term "subjective" is a negative qualifier that should not be used in this context.

Section 2.1.1: It is said that the SEVIRI subregion is 30 W to 30 E and 40 N to 70N. In Figure 7 and A1 to A5 a different region is shown. This is confusing. Which region was taken for the SEVIRI evaluation?

End of page 3: "a very large table": What does that imply? In which sense is the table large and why is it important to emphasize this?

End of page 3: "that can be interpolated ...": to what or between what? The description of what has been done here is not clear enough.

Line 60: "errors of 40 - 60 % in estimated mass loading ...": Which error sources contribute to this overall error? Is it considered that the refractive index of andesite may not be correct for this ash? Which particle shape was considered? Which error in the density of the ash was assumed?

Line 63: I don't like the term "meteorological cloud". I would assume you mean a water

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cloud or an ice cloud. Could you please avoid this term in the whole document?

Line 98: "The exact conversion factor ... was calibrated to match the SEVIRI retrievals ..." What is the interplay between the satellite products? Is one combined product generated? Can the IASI data used independently? (Obviously yes, as it is demonstrated later, but is it in that case calibrated to the SEVIRI data, too?)

Line 108: "We assumed that 10 % of the erupted mass was fine ash ...": What is the basis for this assumption?

Line 112: We all know that the term "reasonable agreement" is difficult because it is not clear what is exactly meant. The situation does not improve by putting it in quotes.

Model simulations: It is not clear why the runs to derive the a posteriori emissions need more disk space "keep the model output at a manageable size) than the run over the whole time period of 41 days. Don't you have to consider all times and layers for the run with the a posteriori emissions, too?

Line 165: "... modeled particle size distributions with a maximum modal diameter below 7 micrometer are inconsistent with downwind in situ measurements of ash particle size distributions ...". I cannot follow that so strictly. Schumann et al. report 12 cases, in 5 of these cases the "diameter of maximum coarse mode volume-size spectrum" (I assume this what you refer to) is below 7 micrometer.

Inversion algorithm: As already said above, the inversion appears to be a "black box". I know that it has already been discussed in other papers but because it is central for the results of this paper, it needs to be explained and discussed more. It particular the reader should be informed if there could be particular difficulties when applying the algorithm to ash particles instead of SO2. Why are the a posteriori emissions lower than before if the satellite images point to higher columnar ash values. Could it be a problem of the algorithm that the emissions in lower altitudes are reduced in the a posteriori case (e.g. is there not enough information from the satellite instruments in

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these altitudes because clouds prevent the observation of ash in lower altitudes or the sensitivity is reduced there?)?

Line 184: "The model results for all scenarios were matched with about 2.3 million satellite observations ...": What does "matched" mean? Is 2.3 million a lot? Does it reduce the uncertainty to have many observations?

A posteriori emissions: Are the results plausible? (From the satellite pictures I would say no). Maybe you could explain a bit what could be expected from the satellite pictures and what the inversion than gave you.

Line 248 and Fig. 4: Since the differences cannot be seen, it is not necessary to show Fig.4.

Line 260: "The main reason for this is that the number of gridded IASI observations is about an order of magnitude smaller than the number of SEVIRI observations, thus providing a weaker constraint on the emissions, which therefore remain closer to the a priori values." Wouldn't it depend in the first instance on the columnar ash values and not on the number of observations how close the a posteriori emissions are to the a priori emissions?

Line 266: "All inversions also lead to substantial emission increases for 12-13 May and to a general shift of the ash emissions to higher altitudes." Isn't it mainly a decrease of emissions in lower altitudes while those in higher altitudes are less influenced? At least for the ECMWF-based inversion with SEVIRI data, there is almost no increase of the emissions in higher altitudes in Fig 5a.

Line 293: "Furthermore, in general, the a posteriori plume heights are in better agreement with the webcam observations than the a priori plume heights." Can that be underlined by some numbers?

Line 320: " ...the comparison with the model is qualitative and further complicated by meteorological clouds, which produce similar backscatter signals as ash clouds."

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Above you said (line 310) "This signal responds to aerosols (including volcanic ash) as well as meteorological clouds which in many cases can be distinguished." Can they be distinguished or is it difficult? How is it done? Can't you use the depolarization signal to distinguish them?

Line 332: "While meteorological clouds often complicate the comparisons, qualitatively we find that the a posteriori results are in better agreement with the CALIPSO data (Fig. A1-A5)." It is not easy to see that. What is your criterion to see it?

Line 333: "We also evaluated our results against quantitative vertical ash concentration profiles obtained from lidar measurements over Europe (Fig. A6-A8) and find that the modeled a posteriori ash concentrations are similar to the observed concentrations." Unfortunately the observations you compare your results with are not shown in the figures. Taking Ansmann's plots, a visual inspection indicates quite some differences, in particular at 13 UT on 16 April the a priori emissions seem to give better results and at the other times, the differences between the different emission cases are rather small. I think your finding is unbalanced towards the a posteriori emissions.

Line 355: "The model captured these ash layers and there is relatively good quantitative agreement between the a posteriori model results and the measurements." Could you give numbers and compare both the a priori and the a posteriori runs to the observations.

Line 368: "The ECMWF-based model sampled along the flight legs with ash ..." What are the results for the GFS run? Why are they not shown?

Line 375: "The data for the comparison was selected by screening the entire observation data set (including gas-phase measurements) for volcanic plumes. An unbiased but imperfect model would underestimate the observations in such a comparison, since slight displacements of the modeled plumes would lead to sampling lower-concentration parts of the plume in the model as compared to the observations." Could you explain this a bit? Couldn't also the aircraft fly in an area of lower ash concen-

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trations compared to a region e.g. 100 km away that is captured by the model? Why should the aircraft always sample the region with the highest concentrations? Line 393: "Patches of highly concentrated ash were present over Europe (10W-30E, 36N-60N) during both April and May, and it is important for aviation to avoid them." How do you know? I think this is nothing you could state here. You can say that according to your model results the limit values were exceeded in some (few) areas but nothing more.

Line 397: "In this paper we have, for the first time, objectively determined the ash emissions of a volcanic eruption as a function of time and altitude." I do not agree that other emission estimates are less objective. Maybe they are less accurate, but your method also includes assumptions that others may call "subjective".

Line 404: " ... ground-based and space-based lidar observations ..." The ground based observations need to be quantified and a figure could be shown in the paper itself if the original data could be plotted in the figures, too.

Figures:

Which heights are given? Are they above sea level or above ground?

Fig. 4 may be omitted. It does not contain new results.

Fig. 7 and A1-A5: Comment also the low clouds in 1-2 km altitude.

Fig 8, Fig. 9: I am quite surprised about the very good timing of the ash cloud in the model and the observations. Is there a reason why this looks almost perfect?

Fig. 11: You could another scatter plots with the GFS results.

Technical corrections

Line 7 "a posteriori model": better "model results with a posteriori ash emissions".

Line 43: on Iceland

Section 2.1.1: something's wrong with the line numbering

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Line 74: "improvement on". Better: improvement compared to

Line 203: "In total, 2.3 million observation cases were used for the inversion." This has been said before.

Line 341: Now "Schumann et al. (2011)".

Figures:

Include the year 2010 in the figures (or captions) where necessary.

Fig.1: A legend fort he thin blue lines would be nice to have.

Fig.5: Use the same dates (every 7 days) as in Figure 2 and 3.

Fig. 6: include a legend for the x-axis

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5541, 2011.